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ON

SIGNOR CARLO MATTEUCCI'S

LETTER

TO

H. BENCE JONES, M.D., F.R.S. &c.

EDITOR OF

AN ABSTRACT OF DR. DU BOIS-REYMOND'S RESEARCHES

IN ANIMAL ELECTRICITY.

BY

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CONTENTS.

	PAGE
INTRODUCTION	5
§ I. SIGNOR MATTEUCCI'S ATTACKS UPON THE AUTHOR	6
II. REMARKS ON THE PERSONAL ATTACKS AND SCIENTIFIC OBJEC- TIONS CONTAINED IN SIGNOR MATTEUCCI'S PAMPHLET .	7
III. GENERAL REMARKS ON SIGNOR MATTEUCCI'S CLAIMS OF PRIORITY	9
IV. HISTORY OF THE DISCOVERY OF THE MUSCULAR CURRENT UP TO SIGNOR MATTEUCCI'S FIRST PAPERS ON THAT SUBJECT	11
V. SIGNOR MATTEUCCI'S RESEARCHES ON THE SO-CALLED FROG CURRENT, AND ON THE MUSCULAR CURRENT UP TO THE TIME OF THE AUTHOR'S FIRST PAPER ON THE SAME SUBJECT	12
VI. STATEMENTS ON THE MUSCULAR CURRENT CONTAINED IN THE AUTHOR'S FIRST PAPER	16
VII. SIGNOR MATTEUCCI'S AND THE AUTHOR'S RESPECTIVE CLAIMS TO THE DISCOVERY OF THE LAW OF THE MUSCULAR CURRENT EXAMINED	19
VIII. FURTHER REMARKS ON SIGNOR MATTEUCCI'S CLAIMS TO THE LAW OF THE MUSCULAR CURRENT	23
IX. ON THE NEGATIVE VARIATION OF THE MUSCULAR CURRENT DURING CONTRACTION	27
X. SIGNOR MATTEUCCI'S SUCCESSIVE STATEMENTS REGARDING THE ELECTRIC DEPARTMENT OF MUSCLES DURING CON- TRACTION	30
XI. SIGNOR MATTEUCCI'S CLAIMS TO THE DISCOVERY OF THE NEGATIVE VARIATION OF THE MUSCULAR CURRENT DURING CONTRACTION EXAMINED	35
CONCLUSION	38

ON
SIGNOR CARLO MATTEUCCI'S
L E T T E R

TO
H. BENCE JONES, M.D., F.R.S.

ETC. ETC.

INTRODUCTION.

SIGNOR CARLO MATTEUCCI, of Pisa, lately addressed a pamphlet* to my friend Dr. Bence Jones, reproaching him for having edited an English translation† of Dr. John Müller's (of Freiburg) Abstract of my Researches in Animal Electricity.‡ Copies of this pamphlet were sent to many scientific men of England and Germany, and, probably, of France.

As the statements contained in the Abstract, of which Signor Matteucci complains, are all taken from my German work,§ I naturally feel bound to answer for them. It would be of no use to do so in German, for in Germany my original work can be procured by every one; and in it such proofs are given in support of these statements (WITHOUT ANY EXCEPTION), as do not admit of the slightest doubt regarding their accuracy. With respect to France, I may also, at least for the present, consider any further discussion with Signor Matteucci as superfluous, since a Committee of the Academy of Paris examined into the

* "Lettre de Charles Matteucci à Mr. H. Bence Jones, F.R.S. &c. &c. Editeur d'une Brochure intitulée 'On Animal Electricity,' ou Extrait de Découvertes de Mr. du Bois-Reymond." Florence: Imprimerie Le Monnier. 1853. 8vo.

† "On Animal Electricity: being an Abstract of the Discoveries of Emil du Bois-Reymond," &c. Edited by H. Bence Jones, &c. London: John Churchill. 1852. 8vo.

‡ "Bericht über die neuesten Fortschritte der Physik." Braunschweig: 1849—1852. S. 768.

§ "Untersuchungen über thierische Elektrizität." Berlin, Verlag von G. Reimer. Bd. I., 1848. Bd. II., Abth. 1, 1849. 8vo.

question three years ago, and gave a verdict by which I am most willing to abide.

In England, on the contrary, the matter is as yet almost new, and requires, perhaps, some elucidation on my part in order to be judged correctly. This information I have endeavoured to give in the following pages. In going over them, the reader, I trust, will remember the fatality naturally attached to the condition of every defendant, namely, that often many pages may be necessary for confuting an accusation expressed in a single line.

As I had no means of knowing who among the scientific men of England were favoured by Signor Matteucci with copies of his pamphlet, I have been obliged to give my reply as wide a circulation as possible by the ordinary way of publication.

§ I. SIGNOR MATTEUCCI'S FORMER ATTACKS UPON THE AUTHOR.

It may be inferred from the preceeding that it is not the first time that I have been the subject of Signor Matteucci's attacks. He has written against me thrice previously, and each time he has been the assailant.

The first time was in 1845.* I had mentioned his name once previously in my first paper of 1843, and I did so very respectfully. I did not answer this first attack explicitly, although it was an exceedingly violent one. Some years later, it is true, I reviewed Signor Matteucci's papers, in the Report of the Physical Society of Berlin,† and in my German work, and of course made the best of my defence. But as these strictures were written in German, they seem to have escaped his attention at the time. It was not until the spring of 1850, when I communicated two papers to the Academy of Paris,‡ in which he was faithfully and respectfully quoted, that he attacked me again in the

* "Annales de Chimie et de Physique." 3^e Série, t. xv., p. 64. "Archives de l'Electricité," par Mr. de la Rive, t. v., p. 382.

† "Die Fortschritte der Physik im Jahre 1845." Dargestellt von der physikalischen Gesellschaft zu Berlin. I. Jahrgang. Redigirt von Dr. G. Karsten. Berlin. 1847. S. 499. See also the same work, II. Jahrgang (1846). S. 436. III. Jahrgang (1847). S. 392.

‡ "Comptes-rendus," &c. 25 Mars et 8 Avril 1850. t. xxx., p. 349 et 406. "Annales de Chimie et de Physique." 3^e Série, t. xxx., p. 119 et 179.

"Comptes-rendus."* I was allowed to answer him in the same journal, and I believe I did so in the most irrefragable manner.† Nevertheless, some time afterwards, Signor Matteucci published a pamphlet in French against me at Florence, very similar to that which is now under consideration, containing, to a great extent, the same personal charges, the same objections to my methods of investigation and to my experimental results, and making the same claims to priority.‡ Copies of it were likewise distributed to many scientific men of Europe. By favour of the Academy of Paris, it was printed in the "Comptes-rendus," notwithstanding its length exceeded the limits prescribed to communications of persons not belonging to the Academy.§ I opposed him again in the same journal with the most forcible arguments,|| and, moreover, the above-mentioned Committee of the Academy decidedly espoused my cause.¶ And yet Signor Matteucci now enters the lists once more, bringing forward the very same charges which I have already so often, and, as I thought, so satisfactorily confuted.

I am desirous of mentioning these facts, because Signor Matteucci does not allude to them in his last pamphlet, so that any one unacquainted with the circumstances might easily be led to suppose that I have provoked this controversy.

§ II. REMARKS ON THE PERSONAL ATTACKS AND SCIENTIFIC OBJECTIONS CONTAINED IN SIGNOR MATTEUCCI'S PAMPHLET.

I do not mean to reply by personal abuse to the language which Signor Matteucci has used.** I shall not at present even

* "Comptes-rendus," &c. 22 April 1850. t. xxx., p. 479.

† "Comptes-rendus," &c. 29 Avril et 6 Mai 1850. t. xxx., p. 512 et 563.

‡ "Réponse aux deux dernières Lettres de M. du Bois-Reymond;" insérées dans les Nos. 17 et 18 des "Comptes-rendus" de l'Académie, et en général à toutes les observations faites par le même auteur sur quelques-unes de mes recherches d'électro-physiologie: présentée à l'Académie des Sciences, par M. Matteucci. Florence: Imprimerie Le Monnier. 8vo.

§ "Comptes-rendus," &c. 3 Juin 1850. t. xxx., p. 699.

|| "Comptes-rendus," &c. 22 Juillet 1850. t. xxxi., p. 91.

¶ "Comptes-rendus," &c. 15 Juillet 1850. t. xxxi., p. 28. The Commissioners were MM. Magendie, Becquerel, Despretz, Rayer, and Pouillet. The latter was the Reporter of the Committee.

** Signor Matteucci, e.g., charges Dr. John Müller, Dr. Bence Jones, and myself, with a *lie* (MENSONGE, p. 8), for having said that "*he always preserved the nervous trunks, and a piece of the spinal column when forming his frog batteries*" (Abstract,

reply again to his scientific objections, but I shall content myself with saying that Signor Matteucci states that these objections are only preliminary ones, as he is going to review the whole of my "Researches" in the new edition of his "Traité" (pp. 3, 14), which he is about to publish. Moreover, as far as he explains himself, he still maintains, as he always did, that my methods of investigation are bad and that my results are wrong. He persists, in short, in contesting nearly all the facts concerning animal electricity, which he has not been fortunate enough to discover himself, as, *e. g.*, the nervous current and the whole world of phenomena which depend on it, (p. 14.) Signor Matteucci thus contests facts which have now been witnessed by the most illustrious physicists and physiologists of England, France, and Germany; facts which were carefully examined and emphatically authenticated by a Committee of the Academy of Paris; facts, indeed, which of late years are demonstrated in public lectures in most German universities, by men like Helmholtz, Ludwig, Bischoff, Vierordt, &c.

I therefore cannot consider it necessary to contend for their accuracy here, or to defend the methods by which these facts were obtained. To give a specimen of the manner in which Signor Matteucci judges of these methods, he ventures to suggest in plain terms, that the only motive why I experiment on a single nerve or muscle of a frog, instead of on batteries formed of twenty legs of frogs, as he did, in imitation of Bunzen* and Nobili,† is that I want to proceed differently from him.

p. 76). He says:—"On peut lire dans mes Mémoires, que je n'ai laissé les filets nerveux dans mes piles musculaires que quand j'ai voulu en étudier l'influence." The passage of the "Abstract" in which Signor Matteucci ventures to point out a *LIE*, forms part of the chapter which explicitly treats of the so-called frog-current, and most evidently does not relate to what he calls "*piles musculaires*," but to what he calls "*piles de grenouilles*." That passage is so perfectly true in reference to the piles de grenouilles, that the nerves and pieces of the spinal column may even be seen in the diagram which Signor Matteucci gives of such a pile in his "*Traité des Phénomènes électro-physiologiques des Animaux*." Paris, 1844. Planché II., fig. 16. He also says in the same work, p. 99:—"Afin d'étudier l'influence des différentes parties de la grenouille dans le courant propre, j'ai commencé par enlever tout-à-fait les deux nerfs spinaux et le morceau de moelle épinière," &c. Hence it follows that the usual mode of preparing the frogs was, to preserve the nervous trunks and the piece of the spinal column, for otherwise he would not have been able to remove them in this particular case.

* Gilbert's "*Annalen der Physik*." 1807. Bd. xxv., S. 155 (1805).

† "*Annales de Chimie et de Physique*." 1828. t. xxxviii., p. 238.

“*La seule raison, qui est bien loin d'être scientifique, par laquelle on fait tant d'efforts pour employer dans les expériences d'électricité musculaire une autre méthode, laquelle consiste principalement à supprimer la pile musculaire et à y substituer un seul morceau de muscle et un galvanomètre excessivement délicat, n'a pu être que celle d'opérer différemment de ce que j'avais fait,*” (p. 6.) I think I need not answer such an assertion. New and valuable results, obtained by means of an experimental method, certainly are the best proof of its excellency; and a great many judges, both competent and impartial, have told me that my methods fairly stood that test.

§ III. GENERAL REMARKS ON SIGNOR MATTEUCCI'S CLAIMS TO PRIORITY.

I now turn to the main subject of Signor Matteucci's letter—viz., to his claims to priority.

Signor Matteucci first denounces the author of the Abstract for purposely omitting to refer to some of his papers anterior to mine, which papers, in his opinion, entitle him to lay claim to some of my experimental results. I venture to assert that such a proceeding would not for a moment have been entertained by Dr. John Müller, Dr. Bence Jones, or me. For my part, I may say that there is scarcely a single line, not only of Signor Matteucci's almost innumerable memoirs on animal electricity, published in the English, French, and Italian periodicals, but even of the whole literature of electro-physiology, which I have not carefully perused, scrupulously weighed, and at last faithfully noted at its exact worth in my German work. Signor Matteucci, who says he has “*écouté la lecture des deux gros volumes de M. du Bois-Reymond,*” (pp. 9, 14, 15,) has had the means of knowing that I have done so.

Signor Matteucci's accusation against the German author and English editor of the Abstract is rather inconsiderate. He says:—“*Vous prétendez dans ce chapitre composer l'histoire de mes travaux d'électro-physiologie, et vous vous arrêtez à l'année 1838, qui est justement l'époque à laquelle mes travaux sur le courant propre et le courant musculaire de la grenouille et sur la contraction induite ont commencé. Malheureusement il n'est que*

trop vrai qu'avant cette époque mes publications étaient trop pressées et imparfaitement étudiées," &c., and after having quoted several papers which Dr. Bence Jones, according to him, ought to have mentioned, he continues:—" *De tout cela vous n'avez pas dit un mot dans le chapitre qui paraît destiné à donner l'histoire de mes recherches,"* (p. 2); and pp. 6, 7:—" *Tout cela a été oublié dans l'histoire que vous faites de mes travaux d'électro-physiologie qui précèdent ceux de M. du Bois-Reymond.*" Now, everybody who will take the trouble of opening the Abstract at p. 95, will find there a whole chapter (Chapter XII., entitled "On the discovery of the law of the muscular current,") solely devoted to a minute analysis of those papers. Signor Matteucci afterwards notes it himself. He says, p. 8: " *Dans le chapitre XII je suis cité plusieurs fois,*" &c. Yet he did not retract his accusation.

Again, Signor Matteucci says, p. 11:—" *Au chapitre XVI il est dit que je n'ai pas prouvé l'existence du courant musculaire dans les animaux vivants: cette assertion est évidemment contraire à la vérité, car j'ai démontré l'existence du courant musculaire sur les grenouilles et sur les pigeons vivants.*" Now, opening the Abstract at the page 125, the reader will find: "Chapter XVI. On the current of the live UNDISSECTED frog. Matteucci long since obtained currents from the DISSECTED muscles of frogs, whilst they were still in connexion with the living body of the animal. But these experiments do not prove, EVEN IN MATTEUCCI'S OWN OPINION, that the current existed in the animal previous TO ANY DISSECTION or experimental arrangement. Du Bois-Reymond has succeeded in demonstrating the current in the live frog WITHOUT ANY DISSECTION." The phrase, "but these experiments do not prove, EVEN IN MATTEUCCI'S OWN OPINION," &c., alludes to the following statement, contained in Signor Matteucci's "Traité," p. 123:—" *Chap. IX. Vues théoriques sur la cause du courant électrique musculaire. Les résultats auxquels nous sommes parvenus sont bien loin de prouver l'existence de l'électricité libre dans les animaux vivants.*"

Such are the foundations on which Signor Matteucci grounds his defamatory assertions against two honourable men, one of whom, it is true, is my countryman, but, beyond that, utterly a stranger to me; the other, it is true, is now my intimate

friend, but not my countryman, and connected with me only by the interest he took in my researches!*

§ IV. HISTORY OF THE DISCOVERY OF THE MUSCULAR CURRENT UP TO SIGNOR MATTEUCCI'S FIRST PAPERS ON THAT SUBJECT.

In order to make the nature of Signor Matteucci's claims better understood, it will be necessary once more to trace here the history of the discovery of the muscular current and of its negative variation during the contraction of the muscle.

The first phenomenon ever observed in reference to the muscular current were the contractions of the limbs of frogs, which Galvani sometimes obtained on completing the circuit between the nerves and muscles, without using metals.† Humboldt noticed similar facts on several animals, both cold and warm-blooded.‡ Volta soon succeeded in more closely determining the conditions requisite for obtaining those contractions. He showed that the nerves should be made to touch the aponeurosis of the tendo Achillis on the back of the gastrocnemius muscle, instead of the muscle itself. The efficacy of this arrangement he explained in accordance with his general theory of electro-motive action by the circumstance of a third heterogeneous body—viz., the tendinous fibre—being interposed between the nerve and the muscle. On making the nerve touch the muscle itself, the limb, according to Volta, will be convulsed only when some contamination, either accidental or intentional, at the place of contact plays the part of the third heterogeneous body, which is requisite for the production

* Here is another instance of the same recklessness. Signor Matteucci reproaches me for not having, in my first paper, described the galvanometer employed (p. 11). Now, on the first page of that paper, I have said—"All my experiments, of course excepting those in which the rheoscopic limb was used, were made with a galvanometer of the usual construction, around the frame of which a copper wire, a kilometer long and .0065 inch of Paris in diameter, forms 4560 coils." And from what follows it may be seen that the sensitiveness of the astatic system was regulated according to Signor Melloni's method of compensation. ("Poggendorff's Annalen," u. s. w. 1843. Bd. lviii., S. 1. Anm.)

† "Dell' Uso e dell' Attività dell' Arco conduttore nelle Contrazioni dei Muscoli." In Bologna, 1794, 4°, p. 82.

‡ "Versuche über die gereizte Muskel-und Nervenfasern," u. s. w. Posen und Berlin, 1797. Bd. i., S. 36, 37.

of the current.* Such was the authority which the invention of the pile soon after bestowed even upon the less correct views of the author, that his opinion prevailed among physicists and physiologists up to our time,† and, in fact, prevented them from investigating the matter further, by shutting their eyes to its real importance.

It was not until 1827, indeed, that Nobili, being then engaged in improving the galvanometer, tried, also by means of this instrument, to detect traces of the current which convulses the frog's limbs in the above-mentioned experiments. He made the feet of a frog, prepared after Galvani's method, dip into one of the conducting vessels of the galvanometer, the sciatic plexuses touched the other, and he observed that the needle was deflected so as to indicate an UPWARD current—that is, a current, directed in the frog's legs, from the feet to the sciatic plexuses (Abstract, p. 68). He moreover found that the intensity of this current, which he called FROG-CURRENT, may be increased by increasing the number of frogs brought into the circuit, thus forming, as it were, a voltaic pile of frogs. As to the origin of the current, Nobili, doubtless ignorant of the peculiar efficacy which Volta discovered in the expansion of the tendo Achillis in Galvani's experiment without metals, strangely conjectured that the frog-current was a thermo-electric current, and arose from the more rapid cooling which he supposed the smaller mass of nerves underwent in comparison with the slower cooling of the larger mass of muscles.‡

§ V. SIGNOR MATTEUCCI'S RESEARCHES ON THE SO-CALLED FROG-CURRENT, AND ON THE MUSCULAR CURRENT UP TO THE TIME OF THE AUTHOR'S FIRST PAPER ON THE SAME SUBJECT.

Nobili's discovery passed almost entirely unnoticed; and the question remained in that state for about ten years longer, when

* "Collezione dell' Opere del Cav. Conte Alessandro Volta." Firenze, 1816. t. ii., p. I., p. 256.

† See Moser in "Dove's Repertorium der Physik." Bd. i., 1837. S. 251.—Prévost, in the "Archives de l'Electricité," &c. 1842. t. ii., p. 633.—Louget, "Traité de Physiologie." t. ii. Paris, 1850. t. ii., p. 130, Note 2.

‡ "Annales de Chimie et de Physique." 1828. t. xxxviii., p. 225; 1830, t. xlv., p. 60.

Signor Matteucci began to work at it. He most undoubtedly has the merit, and I have always given him the credit of having first discerned the importance of the phenomena in question, and the insufficiency of the theories previously proposed, and of having prosecuted their further examination. In 1840, Signor Matteucci published at Paris a compilation of his papers on Animal Electricity, under the title: "Essai sur les Phénomènes électriques des Animaux." The principal step which in this book is made beyond Nobili's range of knowledge consists in the remark, that in order to obtain the frog-current it is not at all necessary to prepare the frog according to Galvani's method, and to connect its nerves and muscles by the galvanometer wire, but that it is sufficient to deprive the frog of its skin and to connect certain points of its body by that wire, as, *e. g.*, its head or back with its legs. The current appears in the same direction as in Nobili's original experiment (Essai, p. 76): "This discovery, as it is said in the Abstract, p. 26, entirely overthrew the old Voltaic doctrine and Nobili's theory of thermo-electricity, as well as the idea of any electro-chemical action."

Signor Matteucci sent a copy of his work to Baron Humboldt. Baron Humboldt gave it Professor John Müller, the celebrated physiologist of Berlin, who gave it to me in the spring of 1841, inviting me to repeat Signor Matteucci's experiments, and, if possible, to extend his researches. Such was the origin of my labours on Animal Electricity. In November, 1842, I communicated a paper to Professor Poggendorff, containing the results I had obtained up to that time. This paper was printed in the "Annalen der Physik und Chemie," Bd. lviii., S. 1, January, 1843, under the title: "A Preliminary Account of an Investigation of the so-called Frog-Current, and of the Electric Fishes."

In the meanwhile, however, Signor Matteucci also had published new observations. In September, 1841, he made the following statement:—*La PARTIE INTERNE D'UN MUSCLE, mise en communication avec une autre partie quelconque du même animal, NERF, SURFACE DU MUSCLE, PEAU, &c., produit un courant qui va dans l'animal de la partie musculaire à la partie qui ne l'est pas.*"* This statement Signor Matteucci usually quotes as the

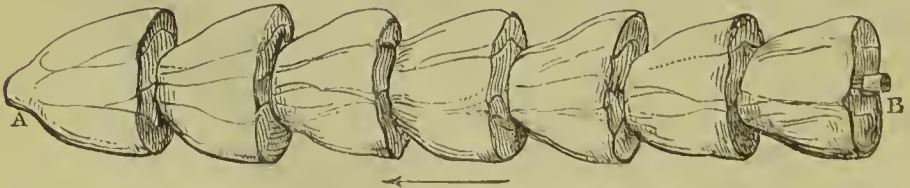
* "Comptes-rendus," &c. 6 Septembre 1841. t. xiii., p. 540.

first in which he gave an account of the law of the muscular current. But the interior and the surface of the muscle, both forming part of the muscle, evidently the direction of the current in the muscle is not unequivocally determined even in that passage.

In February, 1842, Signor Matteucci repeated the same statement, adding, however, some details:—“*De tous les résultats obtenus sur les grenouilles et sur des animaux à sang chaud, on peut tirer cette conclusion : 1° Que la grenouille et les animaux à sang chaud donnent un courant électrique lorsque la partie interne d'une masse musculaire et sa surface sont mises en communication avec un arc conducteur ; 2° Que le nerf qui appartient à une masse musculaire, et tout le système nerveux en général, peuvent faire l'office de la partie interne d'un muscle dans la production de ce courant ; 3° Que le courant est dirigé, dans l'animal, DE L'INTÉRIEUR DU MUSCLE OU DE SON NERF A SA SURFACE OU A SON TENDON.*”* Thus far, Signor Matteucci's new statements were known to me when I published the paper above-mentioned, in which, of course, they are duly quoted and even discussed at length.

These statements were literally taken from an extensive paper, which was afterwards printed at full length in the second volume of the “Archives de l'Electricité,” edited by M. de la Rive, but was not published before the 3rd of November, 1842. Signor Matteucci in this paper first described that experimental arrangement which he has since called “*pile musculaire*,” and which he still considers as most appropriate for the demonstration of the electric properties of muscles. The *pile musculaire* consists of a certain number up to twenty thighs of frogs, roughly cut across, and disposed in the manner represented in the annexed diagram. By connecting the two ends of the

FIG. 1.



[Simplest case of the Muscular Current observed by Signor Matteucci, up to January, 1843.
Published November, 1842.]

* “Comptes-rendus,” &c. 21 Février 1842. t. xiv., p. 315.

battery—viz., the stump of the knee in *a*, and the cross section of the thigh in *b*, by the galvanometer wire, a current is obtained in the direction of the arrow.*

This current was considered by Signor Mattencci as an entirely new phenomenon, quite distinct from the frog current;—nay, so wholly uneonneeted with it, that he admitted that the muscular current was found in all animals, including the frog, whereas the frog current is said by him to belong to the frog exclusively. He seems to have ascribed to this animal, although unprovided with any peculiar organ for that purpose, an electromotive power similar to that of the electric fishes.† Even as late as 1844, Signor Matteucci, after having developed some hypothetieal views concerning the muscular current, says: “*Ces vues hypothétiques ne peuvent pas malheureusement s’étendre aussi bien aux phénomènes du courant propre de la grenouille. J’AI CHERCHÉ INUTILEMENT L’EXISTENCE D’UN COURANT ANALOGUE AU COURANT PROPRE DE LA GRENOUILLE, DANS UN TRÈS GRAND NOMBRE D’ANIMAUX, je n’ai jamais trouvé que le courant musculaire. Les animaux les plus rapprochés de la grenouille, des salamandres, des anguilles, des tortues, ne m’ont jamais donné que le courant musculaire; dans tous les cas, il a fallu invariablement, pour obtenir les signes d’un courant électrique, avoir dans le circuit l’intérieur du muscle et sa surface. DANS LA SEULE GRENOUILLE ON TROUVE EN MÊME TEMPS LE COURANT MUSCULAIRE ET LE COURANT PROPRE qui existe, sans altérer le muscle, sans mettre à nu sa partie interne, et qui est dirigé de la surface du muscle à son nerf dans l’animal.*”‡ Signor Matteucci, in these papers, therefore also admits the possibility of the two currents—viz., the frog and the muscular current—counterbalaneing each other in the frog’s leg, so that at one time the former, at another the latter, may prevail in the circuit.§

* “Archives de l’Electricité,” t. ii., pp. 434—436.

† “Essai sur les Phénomènes électriques des Animaux.” Paris, 1840. p. 84.

‡ “Traité des Phénomènes électro-physiologiques des Animaux.” Paris, 1844. pp. 126, 127.

§ Signor Matteucci, in his last pamphlet, p. 8, appeals to Galvani, Humboldt, and Nobili, as having likewise considered that the frog current was a phenomenon exclusively belonging to the frog, in order to exonerate himself for having once assented to that opinion. But in doing so he is altogether mistaken. Nobili, who ascribed a thermo-electric origin to the frog current, of course did not even consider it as a phenomenon confined to the animal kingdom, much less to a single species. As regards Galvani and Humboldt, almost every line of their writings is sufficient to prove that throughout their experiments on frogs they tried to detect general facts,

§ VI. STATEMENTS ON THE MUSCULAR CURRENT CONTAINED
IN THE AUTHOR'S FIRST PAPER.

Such were Signor Matteucci's ideas concerning the muscular and the frog current, at the time when my first paper appeared in print, and even much later. The statements on the same subject contained in that paper are the following:—

1. Currents, in all respects similar to the so-called frog current, may be observed in any limb of any animal, whether cold or warm-blooded. These currents, in some limbs, are directed upwards, as in the frog's legs—in others, downwards. They are of different intensity in different limbs. But their intensity and direction are always the same in the same limb of different individuals of the same species.

2. The electro-motive action on which these currents depend does not arise from the contact of heterogeneous tissues, as Volta supposed, for the different tissues, the nerves, muscles, and tendons, in an electric point of view, are quite homogeneous.

3. These currents are produced by the muscles. If any undissected muscle of any animal be brought into the circuit longitudinally, it generally exhibits an electro-motive action, the direction of which depends on the position of the muscle on the ends of the galvanometer circuit, according to the law which will immediately be stated. Thus, the current might be a downward one, or it might be an upward one.* The current of

applicable to the muscles and nerves of all animals. Humboldt accordingly extended Galvani's experiment without metals to the *Rana temporaria*, *Hyla arborea*, *Lacerta agilis*, *Mus musculus* (see above, p. 11). Galvani himself, it is true, in his experiments without metals, seems never to have forsaken that animal to which he owes the immortality of his name. But Aldini, Valli, and many others made experiments with other animals without ever mentioning that by doing so they passed beyond the limits of Galvani's original ideas. The strange opinion, therefore, that the *Rana esculenta* is an electric animal comparable to the *Gymnotus* and the *Torpedo*, solely owes its origin to Signor Matteucci, and I, in opposing it, only restored the investigation to its former true direction.

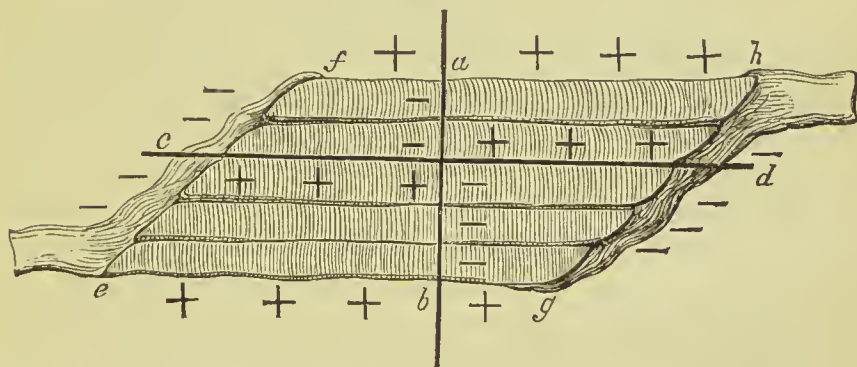
* Signor Matteucci, when enumerating in his last pamphlet the mistakes with which, according to him, my first paper abounds, also says:—"Il est dit que certains muscles de lapin et de pigeon et de la grenouille même, donnent des courants de direction contraire à celui de la jambe de la grenouille" (p. 11); and:—"M. du Bois-Reymond, dans son Mémoire de 1843 et dans ses deux volumes publiés longtemps après, a trouvé bien des anomalies dans la direction des courants obtenus des différents muscles entiers de la grenouille" (p. 10). It is difficult to understand how Signor Matteucci, who asserts that he observed the law of the muscular current, can contest a fact so easily demonstrated by experiment, and so simple to explain by theory. See further on, section 5, p. 18.

the whole limb is nothing but the resultant of the partial currents which are engendered by each muscle of the limb; and the frog current as well as the similar currents observed in other animals, are thus simply reduced to a general MUSCULAR CURRENT. I THEREFORE INVENTED AND FIRST USED THIS TERM, AND IT DOES NOT OCCUR IN SIGNOR MATTEUCCI'S PAPERS ANTERIOR TO MINE.*

4. To make the law, according to which this current may be regularly obtained, more easily understood, it will be useful to premise some definitions.

By LONGITUDINAL SECTION of the muscle, I understand a surface formed only by the SIDES of the fibres of the muscles considered as prisms. By TRANSVERSE SECTION of the muscle I likewise understand a surface formed by the BASE of the fibres of the muscles again considered as prisms. Both the transverse and the longitudinal section may be either ARTIFICIAL or NATURAL ones. In fig. 2, which of course is an ideal one,

FIG. 2.



a section of the muscle through *a b* would be an artificial transverse one; a section through *c d* an artificial longitudinal one. As to the natural transverse section, it is at each end of the muscle formed by the ends of all the fibres, and hidden beneath a coating of tendinous tissue, which is in connexion with the tendon itself, and, in an electric point of view, plays the part of an indifferent conducting body, (*e f, g h* in the diagram.) Thus, the above-mentioned aponeurosis of the tendo-Achillis on the back of the gastrocnemius muscle

* It is first used by him in the "Comptes-rendus," &c. 23 Janvier 1843, t. xvi., p. 197.

covers the under natural transverse section of that muscle. Finally, the natural longitudinal section of the muscle is that part of its external surface which extends from one natural transverse section to the other, being free from the tendinous coating, and exhibiting the red colour peculiar to muscles, (*fh*, *eg* in the diagram.)

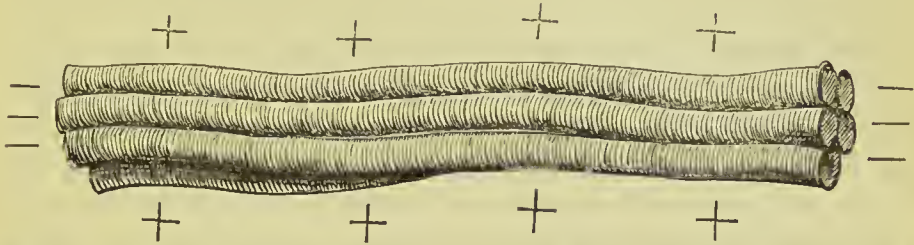
This being thus stated, the law of the muscular current* may be shortly expressed as follows:—ANY POINT OF THE NATURAL OR ARTIFICIAL LONGITUDINAL SECTION OF THE MUSCLE IS POSITIVE IN RELATION TO ANY POINT OF THE NATURAL OR ARTIFICIAL TRANSVERSE SECTION. (See the algebraic signs in fig. 2.)

5. This very important law implied and explained at once all the cases of electro-motive action previously observed in muscles, including Signor Matteucci's latest statements (which, as I have already said, are quoted in my paper as far as I knew them), as well as Volta's long-forgotten remark concerning the peculiar efficacy of the tendon in Galvani's experiment. By means of the above-mentioned law it is very easy to understand why the muscular current in one instance appears to be an upward, in another a downward one; according as the under or the upper of the two transverse sections is made to touch one of the ends of the galvanometer wire, whilst the other end is applied to the longitudinal section of the muscle. Again, according to that law, every particle of a muscle, however minute, ought to produce a current in the same manner as the whole muscle, or as a larger piece of it. This consequence in my first paper is shown to be true even as regards shreds of muscle, consisting

* Signor Matteucci also is accustomed to speak of "the laws of the muscular current." But hitherto he did not mean by these laws, as I do, the general and condensed expression of the conditions, according to which a given muscle may be made to display its electro-motive action, but the detailed statement of the influence which several circumstances, generally of a deleterious or injurious character, exert upon the electro-motive power of muscles ("Comptes-rendus," &c. 3 Juin 1850. t. xxx., pp. 700, 701. "Traité," &c., 1844, pp. 58, 67). My manner of using the term in question is, however, plainly the more correct of the two; and accordingly it not only prevails among German writers, but it is also sanctioned by the Committee of the Academy of Paris ("Comptes-rendus," &c. 15 Juillet 1850. t. xxxi., p. 46). Signor Matteucci himself, in the pamphlet under review, seems to have yielded to some consideration of this kind; for he now, very incautiously indeed, indifferently uses the term in both acceptations (p. 13).

only of a few primary fibres, and such as, accordingly, admit of microscopical investigation, as shown in fig. 3.*

FIG. 3.



[Simplest case of the Muscular Current observed by Dr. du Bois-Reymond up to January, 1843. Published January, 1843. The primary fibres are magnified 75 times.]

6. As to the nerves, I have stated in my paper that they are possessed of an electro-motive power, which acts according to the same law as the muscles. Whilst still in organic connexion with the muscles, and forming part of a circuit in which the muscles give rise to a current, the nerves simply play the part of an inactive conducting body, provided their own current be prevented from entering the circuit.

§ VII. SIGNOR MATTEUCCI'S AND THE AUTHOR'S RESPECTIVE CLAIMS TO THE DISCOVERY OF THE LAW OF THE MUSCULAR CURRENT EXAMINED.

In order to facilitate the comparison of Signor Matteucci's statements and those contained in my first paper, I have put both into the following table :—

SIGNOR MATTEUCCI'S STATEMENTS IN NOVEMBER, 1842.

There are two distinct currents, namely—

1. *The Frog Current.*

The leg of the frog is endowed with an upward electro-motive power, peculiar to this animal alone.

2. *A New Current, not yet named.*

In the frog and in warm-blooded animals, the surface of the muscle, including the expansion of the tendon, is positive in relation to the interior of the muscle, the nerve which ramifies in the muscle, and the whole nervous system.

DR. DU BOIS-REYMOND'S STATEMENTS IN JANUARY, 1843.

There is but one current, namely—
The Muscular Current.

Any part of the natural or artificial longitudinal section of any muscle of any animal is positive in relation to any point of the natural or artificial transverse section.

The nervous system, setting aside its own electro-motive power, which obeys the same law as the muscles, plays the part of an inactive conductor with respect to the muscular current.

* Signor Matteucci, who still considers that a pile of twenty thighs of frogs, as shown in Fig. 1, is the best contrivance for demonstrating the muscular current, has ridiculed the statement contained in my first paper, concerning the possibility of

Such, then, was the state of knowledge on both sides at that time. Signor Matteucci, it is true, first pointed out several experimental arrangements in which the artificial transverse section of muscles exhibited a negative electric state in relation to other parts of the animal. His experiments, however, were not even made on simple muscles, but on whole limbs of frogs, including bones, ligaments, tendons, nerves, large bloodvessels, &c.; whereas he never attempted to prove that these different tissues are not heterogeneous in an electric point of view; nay, whereas in his third attack upon me he explicitly owns:—“*Je ne connais pas une expérience qui me prouve que le tendon n'est et ne peut être autre chose qu'un simple conducteur de l'état électrique de la partie intérieure du muscle.*”*

Signor Matteucci, most undoubtedly, was on the threshold of the discovery of the law of the muscular current. So was Signor Grimelli, who nearly at the same time proved that Galvani's experiment without metals succeeds much better on making the sciatic nerve touch the artificial transverse section (*sezione trasversale*), instead of the external surface of the gastrocnemius muscle.† So was, in truth, old Volta, too, when he pointed out the peculiar efficacy of the aponeurosis of the tendo-Achillis, that is, of the under natural transverse section of the gastrocnemius muscle in Galvani's experiment. They all had met with some particular instance of electro-motive action displayed by the muscle, but none of them—and Signor Matteucci no more than any one else—succeeded previously in reducing those par-

observing the muscular current even on such microscopical shreds of muscle (“*Comptes-rendus*,” &c., 3 Juin 1850, t. xxx., p. 704). After having shown this experiment to most physicists and physiologists of England, France, and Germany, I need not insist on its possibility here. In his last pamphlet, however, Signor Matteucci seems to have erred another way with respect to the same point. He says:—“*D'abord, ET POUR ETRE VRAI, au lieu d'un morceau microscopique il faut désormais entendre qu'on parle d'un morceau de muscle qui est employé à fermer le circuit du galvanomètre*” (p. 10). He, therefore, now seems to think that I have boasted of having performed experiments on microscopical shreds of muscle, whilst, in truth, these experiments were made with whole muscles. I cannot understand how Signor Matteucci was led into this new mistake. A short account of my experiments on microscopical shreds of muscle is given in the “*Abstract*,” p. 100.

* “*Réponse aux deux dernières Lettres de M. du Bois-Reymond*,” &c. Florence. 1850. p. 4. See above, p. 12, note 2, several quotations to the effect of showing that the different tissues are generally considered to be heterogeneous in an electric point of view.

† “*Giornale Letterario-Scientifico Modenese*.” Fascicolo di Aprile e Maggio, 1842.

ticular instances to one general principle, which not only explained all the cases ever observed before, but even led the way to cases yet unknown.

Signor Matteucci, at the time when my paper was published, had no idea of the negative condition of the natural transverse section of muscles. He supposes that its electric condition is the opposite of that of the artificial transverse section, and that it is the same as that of the natural longitudinal section, whilst, in fact, it is just the contrary.

He, further, had no idea whatever of the electric condition of the artificial longitudinal section. Signor Matteucci, indeed, persists in contending that it is quite indifferent whether the artificial transverse section be so called, as I have proposed, or be called interior of the muscle, as he proposes. But by doing so he clearly shows that even now he does not thoroughly understand that law of the muscular current, which he professes that he discovered. The interior of the muscle, evidently, comprehends the longitudinal section as well as the transverse one. According to Signor Matteucci's statement, therefore, the artificial longitudinal section ought to be found in a negative condition when compared to the natural longitudinal section. I need hardly repeat that such is not the case. The two kinds of longitudinal sections are perfectly homogeneous. Signor Matteucci thus wrongly defined the electric condition of the muscles as if they were comparable to a mass of copper thoroughly coated with zinc; whilst I, in my first paper, in fact, compared the muscles to a bundle of very thin copper wires, the cylindrical surfaces of which were coated with zinc, whilst the bases were left uncovered. And yet Signor Matteucci unhesitatingly writes in his last pamphlet:—"*Toutes mes expériences sur le courant musculaire que j'ai publiées dans mes Mémoires de 1838 à 1842, ET AUXQUELLES ON N'A RIEN CHANGÉ DEPUIS,*" &c.—(p. 10.)

Again, Signor Matteucci asserts that the nerve which ramifies in a muscle (nay, the whole nervous system) partakes of the negative electric state wrongly ascribed by him to the interior of the muscle. On connecting the nerve and the tendon by the galvanometer wire, a current is said by him to be obtained which proceeds from the former to the latter in the animal. This

pretended current he strangely identifies with the muscular current. I need not say these statements are altogether wrong.*

The reader is now, at length, fully acquainted with the state of the question, and enabled to judge of the justness of Signor Matteucci's complaints. Signor Matteucci is not contented with the fact that I have, on every occasion, proclaimed that he first pointed out the negative electric condition of the artificial transverse section. He further claims the discovery of the law of the muscular current; and yet

1. His statements concerning that law, anterior, nay, even those posterior to mine by some years, have been shown by me to be altogether incomplete and wrong.

2. He does not, even now, rightly understand that law, for he persists in the statement, that the interior of the muscle is negative in relation to its surface.

3. He does not, even now, implicitly grant the identity, in an electric point of view, of the artificial transverse section, and of the expansion of the tendon: "*Il m'est impossible d'admettre sans réserve l'identité du rôle du tendon avec celui de l'intérieur du muscle.*"—(pp. 10, 11.)

4. But, at all events, he explicitly and repeatedly grants that I first pointed out that identity: "*C'est M. du Bois-Reymond qui a publié le premier que le courant soi-disant propre de la grenouille existait dans les muscles entiers d'autres animaux, à la condition de faire arc entre la surface du muscle et son tendon.*"—(p. 5.) And: "*Il a trouvé dans les muscles entiers d'autres animaux, outre ceux de la grenouille, des phénomènes plus ou moins analogues à celui du courant propre, à la suite*

* Signor Matteucci, it is true, says, in his pamphlet, p. 9:—"J'ai eu pourtant soin de décrire dans mon *Mémoire* de 1842, les expériences qui prouvent que les filets nerveux se comportent comme des simples conducteurs, semblables à un fil de chanvre ou à un morceau de papier imbibé d'eau;" and further: "*En citant mon Mémoire de 1842, je viens de rappeler que je ne m'étais pas trompé sur le rôle des nerfs dans les expériences d'électricité musculaire.*" I am sorry to state that Signor Matteucci's memory failed him when he wrote those lines. His first correct statements on the inactive part which the nerves play in the experiments on the muscular current, were published in April, 1843, three months after my first paper. An error of date of such importance, I should think, ought not to occur in writings like Signor Matteucci's pamphlet. But this is, moreover, the second time that he has committed this same error. It is also to be found in his third attack upon me ("*Comptes-rendus,*" &c., 3 Juin 1850, t. xxx., p. 701). I cannot refrain from asking what abuse would have been poured upon my friend, Dr. Bennet Jones, if he had stated that my first paper appeared in print in 1842 instead of in 1843.

desquels il a été conduit à admettre que les extrémités tendineuses des muscles jouent le rôle dans les electro-moteurs musculaires de l'intérieur de ces tissus. Quoiqu'à cette époque, comme mes Mémoires le prouvent, j'eusse déjà, comme M. Cima de son côté l'avait fait, trouvé cette généralisation du courant propre de la grenouille, je n'ai pas manqué de déclarer que M. du Bois-Reymond avait publié cette découverte le premier." (p. 15; see passages to the same effect at pp. 8 and 11.)

That identity, however, which Signor Matteucci still contests, and, accordingly, so readily yields up its discovery, most evidently was the very thing to be discovered and explained here, and it was, as it were, the very key to the law of the muscular current. So I confidently may leave it with my reader to draw the right conclusion from these premises. His inference, I trust, will be the same as has already been drawn by the scientific men of Germany, and by the Academy of Paris.*

§ VIII. FURTHER REMARKS ON SIGNOR MATTEUCCI'S CLAIMS TO THE LAW OF THE MUSCULAR CURRENT.

Before leaving this point of the controversy, I beg to add a few more remarks.

1. I cannot forbear once more calling to mind that, setting aside my German publications altogether, I three years ago brought forward very nearly the same arguments as those above in the "Comptes-rendus," in reply to Signor Matteucci's second attack. (See above, p. 7, note 2.) He, of course, was at liberty not to feel satisfied with my defence. But unless he had preferred dropping the controversy altogether, which no doubt would have been the best plan, he ought to have discussed my arguments one by one, and to have shown where they were

* If the law of the muscular current had not been published by me in January, 1843, Signor Antonio Cima, of Cagliari, would be most entitled to its discovery. That philosopher delivered a paper to the Academy of Bologna in December, 1844, in which the undissected muscles of all animals are shown to be possessed of currents like the so-called frog current, and the identity, in an electric point of view, of the artificial transverse section and of the expansion of the tendon, is unequivocally proclaimed. (Antonio Cima, "Saggio storico-critico e sperimentale sulle contrazioni galvaniche o sulle correnti elettro-fisiologiche" in Signor Zantedeschi's "Raccolta fisico-chimica Italiana," &c. 1848. Vol. iii., p. 421.) This paper, as will soon be seen, is three months anterior to the paper of Signor Matteucci in which the same statements are contained. Signor Matteucci himself, in his last pamphlet, grants to Signor Cima the prior claim to these statements. See above, p. 23.)

wrong. Instead of doing so, Signor Matteucci, after having kept silence for some years, now brings forward just the same assertion of priority, without mentioning a single word regarding the reply which I formerly made to him on the same subject.

2. Signor Matteucci, as late as 1845, continued to repeat the erroneous statements contained in his paper of November, 1842. They were reprinted at length in his "*Traité des Phénomènes électro-physiologiques des Animaux*," Paris, 1844; only in this book the nerves are said to play the part of inactive conductors. In the report of the fourteenth meeting of the British Association, held at York in September, 1844, it is said:—"*M. Matteucci explained the specific voltaic current (courant propre) of the frog as being a current which is detected only in the frog, and which is directed from the feet to the head of the animal.*"*

In April, 1845, at last, more than two years after my first paper had been published, Signor Matteucci wrote in the "*Comptes-rendus*" as follows:—"*Les résultats les plus curieux, auxquels je suis parvenu dans ces derniers travaux, sont relatifs au courant propre de la grenouille. Je puis maintenant affirmer que ce courant n'appartient pas exclusivement à la grenouille, mais qu'il se manifeste dans tous les muscles de tous les animaux, pourvu que ces muscles présentent à leurs extrémités une terminaison tendineuse inégale. . . . Si j'ai bien compris les derniers travaux anatomiques faits sur la structure des muscles, sur ses rapports avec les tendons et le sarcolème, je ne puis pas hésiter à regarder le courant propre ou du tendon à la surface du muscle, comme le cas le plus simple du courant musculaire.*"† About the same time, Signor Matteucci wrote in the "*Philosophical Transactions of the Royal Society*" as follows:—"*But why should the proper current belong exclusively to the frog? This is the problem the solution of which I have long been anxious to arrive at, and hope finally to have given a satisfactory answer. . . . The following are the experiments which led me to generalize the fact of the proper current.*"‡ . . . And then follow some experiments

* "*Transactions of the Sections*," p. 38.

† "*Comptes-rendus*," &c. 14 Avril 1845. t. xx., p. 1098.

‡ "*Philosophical Transactions for the year 1845*," P. ii., pp. 298, 299.

and considerations, to the effect of showing that currents, similar to the so-called frog current, may be obtained from the muscles of other animals, and that the expansion of the tendon, in an electric point of view, may be considered as representing the interior of the muscle.

In these papers my name is not mentioned. In his last pamphlet, however, Signor Matteucci explicitly declares:—*“ Dans mes Mémoires publiés dans les Philosophical Transactions (1845), j’ai décrit les expériences faites avec des piles de muscles entiers pris sur différens animaux, et ces expériences ont démontré que ce phénomène appelé jusqu’alors du courant propre, existait dans tous les animaux, et que l’extrémité tendineuse fonctionnait comme l’intérieur du muscle. Il est vrai qu’à cette époque je connaissais le Mémoire que M. du Bois-Reymond avait publié en 1843. . . C’est par la lecture de ce Mémoire que j’ai été amené à étudier avec ma méthode l’existence et les lois du courant électrique appartenant aux muscles entiers de tous les animaux.”*—(p. 11.)

These facts need no comment. Signor Matteucci confessedly knew my paper, in which the existence of currents similar to the frog current in the undissected muscles of all animals, and the identity, in an electric point of view, of the expansion of the tendon and of the artificial transverse section are most fully stated; and yet, WITHOUT MENTIONING MY NAME, he wrote these lines:—*“ This is the problem the solution of which I have long been anxious to arrive at, and hope finally to have given a satisfactory answer.”* He now confesses:—*“ C’est justement par la lecture de ce Mémoire que j’ai été amené à étudier l’existence du courant électrique appartenant aux muscles entiers de tous les animaux;”* and yet, in May, 1845, WITHOUT MENTIONING THAT PAPER, he wrote—*“ The following are the experiments which led me to generalize the fact of the proper current.”*

In September, 1845, a letter of Signor Matteucci, directed to M. Dumas, was printed in the *“ Annales de Chimie et de Physique.”* In this letter he for the first time attacked me, and also Professor John Müller (of Berlin)—the latter, for having, in his *“ Elements of Physiology,”* after a faithful statement of Signor Matteucci’s researches, ascribed to me the discovery of

the law of the muscular current.* Signor Matteucci in that letter casually says, and in a rather depreciating tone:—“*Il est juste de dire que, quoique d'une manière incomplète, il a le mérite d'avoir indiqué le premier le rôle de la substance tendineuse dans le courant propre tel que je viens de l'indiquer dans mon dernier Mémoire.*”†

According to his recent avowal, he at least ought to have said, that he was indebted to me for the fundamental idea of that paper. Yet, on the ground of that passage, Signor Matteucci says in his last pamphlet:—“*Je tiens aussi à vous faire remarquer que je n'ai pas manqué de FAIRNESS en déclarant publiquement que c'était M. du Bois-Reymond qui avait publié le premier que le courant soi-disant propre de la grenouille existait dans les muscles entiers d'autres animaux, à la condition de faire arc entre la surface du muscle et son tendon.*”—(p. 5.)

Signor Matteucci forgets that he has forfeited the merit of that apparent concession—1. By publishing in the “*Annales de Chimie et de Physique*” for August, 1846,‡ a translation of his paper in the “*Philosophical Transactions*,” WITHOUT MENTIONING MY NAME; 2. By writing to M. Dumas, in August, 1846, WITHOUT MENTIONING MY NAME:—“*J'ai été satisfait d'être parvenu à démontrer que le courant propre existe dans tous les animaux, qu'il est dû à une structure particulière que j'ai déterminée des faisceaux musculaires (?), et qu'il n'est, suivant toutes les probabilités, qu'un cas de courant musculaire;*”§ 3. By communicating the same statement to the British Association at Southampton, in September, 1846,|| WITHOUT MENTIONING MY NAME; and 4. By stating in his “*Leçons sur les Phénomènes physiques des Corps vivants*,” Paris, 1847, pp. 211, 212, WITHOUT MENTIONING MY NAME:—“*Récemment, en étudiant plus attentivement le courant propre, j'ai pu m'assurer que c'était un phénomène qui appartenait à tous les animaux. . . . Ayant fixé ainsi les conditions desquelles dépend le courant propre, je crois pouvoir en généraliser l'origine et le réunir au courant musculaire,*” &c.—in short,

* “*Handbuch der Physiologie des Menschen.*” Bd. i., 4. Auflage. Coblenz, 1844, S. 557.

† “*Annales de Chimie et de Physique.*” 3^e Série. t. xv., p. 67.

‡ 3^e Série. t. xviii., p. 114.

§ “*Comptes-rendus,*” &c. 17 Août 1846. t. xxiii., pp. 356, 357.

|| “*Transactions of the Sections,*” p. 8. “*The Athenæum,*” No. 987, p. 998.

by repeatedly proclaiming as his own discovery facts which he now confesses that he derived from my paper.

3. Lastly, although Signor Matteucci, in September, 1844, still maintained at York the specific nature of the so-called frog current (see above, p. 24), and although he, at page 11 of his last pamphlet, owns that it was by reading my paper of January, 1843, that he first, in 1845, was led to generalize the fact of that current, he nevertheless, at page 5 of that same pamphlet, says:—“*Il suffit de lire mes Mémoires d'électro-physiologie de cette époque (de 1838 à 1842), pour acquérir la certitude, qu'avant de connaître le Mémoire de M. du Bois-Reymond j'étais parvenu à trouver sur d'autres animaux le courant qu'on* (that is to say, nobody excepting Signor Matteucci) *avait cru d'abord propre de la grenouille seule.*” And at page 15: “*Quoiqu'à cette époque* (that is, at the time when he first read my paper), *comme mes Mémoires le prouvent, j'eusse déjà, comme M. Cima de son côté l'avait fait, trouvé cette généralisation du courant propre de la grenouille,*” &c.

I again abstain from any comment on these facts.

§ IX. ON THE NEGATIVE VARIATION OF THE MUSCULAR CURRENT DURING CONTRACTION.

I now turn to the second subject of Signor Matteucci's claims.

Since the middle of the last century, innumerable attempts have been made to detect any electric phenomenon exhibited by the muscles during contraction. Signor Matteucci himself had made some random experiments on that subject.* They were all made in vain.

In the summer, 1842, as soon as I had mastered the electric phenomena of muscles when at rest, by the discovery of the law of the muscular current, I also turned my attention to that most important problem, and, I am told, was fortunate enough to solve it satisfactorily.

I simply put a muscle into the circuit of the galvanometer, and waited until the needle took up its new position of equilibrium under the influence of the muscular current, whatever

* “*Essai sur les Phénomènes électriques des Animaux.*” Paris, 1840. p. 36.

its direction in the muscle might be, in consequence of the position of the muscle on the ends of the galvanometer circuit, and in accordance with the law of the muscular current. I then proceeded to tetanise the muscle—that is, to make it contract powerfully and uninterruptedly as long as possible, in order to protract the action, and therefore to increase the effect exerted upon the needle by any change which the electric condition of the muscle might undergo during the contraction. In order to effect this the muscle was still attached to its motor nerve, which, however, did not form part of the circuit of the galvanometer, and to which, therefore, any stimulus could be applied without endangering the equilibrium of the needle otherwise than by the very effect to be observed—namely, by any electro-motive action taking place during the contraction of the muscle.

The result was, that at the moment when the muscle was convulsed a sudden and considerable decrease of its action on the needle occurred. This decrease lasts as long as the tetanus itself, and cannot be accounted for by any change of the resistance of the circuit, for it is also obtained when there are two muscles in the circuit, the currents of which counterbalance each other; in this case no perturbation of the electric equilibrium can ever arise from any change of the resistance of the circuit.

Thus far my results on that subject were published in my paper of January 1843 (pp. 12, § 32). It is well known that I have since extended those experiments successfully to the living body of the frog, of the rabbit,* and even of man (Abstract, chap. xix.), and that I have detected a negative variation† of a similar description which the nervous current undergoes while the nerve is conveying to the brain, or to the muscle, those material changes which give rise to sensation and motion. (Abstract, chaps. xxv.-xxx.)

It is well known, too, that by the negative variation I have

* "Monatsberichte der Berliner Akademie." Januar 1853. S. 118.

† Instead of "decrease," I usually say "negative variation" of the muscular current during the contraction, because as yet I have not been able to make out whether during contraction there is only a decrease in the intensity of the current, or whether the direction of the current is reversed. (Abstract, p. 147.) In the following remarks the phenomenon in question, for the sake of brevity, will be called simply—"The negative variation."

succeeded in explaining that interesting phenomenon, first pointed out by Signor Matteucci in October, 1842,* namely, the contraction sometimes shown by the limb of a frog B, called the RHEOSCOPIC LIMB, the nerve of which is placed in any position on another limb A, when the latter is made to contract.

Signor Matteucci considered that contraction as "*a first fact of induction of that force which circulates in the nerves, and which arouses muscular contraction,*" and he therefore adopted for it the name, casually bestowed upon it in England, of "*the Induced Contraction.*"†

I, on the contrary, proved by the most striking experiments, that the so-called "Induced Contraction" is nothing but the common physiological effect which the negative variation of the muscular current of the primary contracting muscle A exerts on the nerve of the rheoscopic limb B, in accordance with my general law of the excitation of nerves by an electric current. (Abstract, chap. vii.) The nerve of the rheoscopic limb, therefore, ought to be placed on the limb A, so as to complete the circuit between the longitudinal and the transverse section of the muscle, that is to say, so as to be pervaded by the muscular current, otherwise no "Induced Contractions" will occur. As, then, there is no nervous induction acting at a distance here, as Signor Matteucci strangely conjectured, and as, after having given the theory of the phenomenon, I might well feel entitled to alter its name according to the more correct views I had introduced respecting its nature, I ventured to call it "Secondary Contraction" instead of "Induced Contraction," as it was miscalled by its discoverer.‡

Signor Matteucci, in his last pamphlet, says I did so, "*pour lui donner une dénomination différente de celle que j'avais adoptée*" (p. 12). He contests the general law of the excitation of nerves by an electric current (p. 7). He still denies that it is necessary, in order to obtain the secondary contraction, that the nerve of the rheoscopic limb should be traversed by the muscular current of the primary contracting muscle (p. 13).

* "Comptes-rendus," &c. 17 Octobre 1842. t. xv., p. 797.

† "Philosophical Transactions for the year 1845," P. ii., p. 303.

‡ "Abstract," Chapter XVIII. This explanation of the secondary contraction was first printed in Professor John Müller's (of Berlin) "Handbuch der Physiologie des Menschen." Bd. i., 4. Auflage. Coblenz, 1844, S. 557.

He, moreover, still refuses to admit even the electric origin of the secondary contraction (p. 13), and he persists in asserting that the rough and complicated form under which the secondary contraction first occurred by accident, whereby its true nature could not be perceived, is the most simple and most appropriate possible (pp. 12, 13).

With respect to these assertions I can but repeat here what I have already said at the commencement (see above, p. 8), namely, that I cannot deem it necessary again to contend for the accuracy of facts, and for the correctness of views which in France are sanctioned by the Academy of Paris,* and form part of the public instruction in Germany, unless new experiments be adduced against them. As long as Signor Matteucci will content himself with repeating again and again the same statements from time to time, without in the least regarding the arguments against them published by me years ago, I on my part shall not, I hope, be blamed for not taking further notice of his assertions.

§ X. SIGNOR MATTEUCCI'S SUCCESSIVE STATEMENTS REGARDING THE ELECTRIC DEPARTMENT OF MUSCLES DURING CONTRACTION.

In his paper of 1838, literally reprinted in the "Essai" (1840), Signor Matteucci said:—"Une autre cause qui modifie grandement le courant propre de la grenouille, c'est son état tétanique. . . . L'influence du tétanos est telle que le courant propre manque toujours lorsque la grenouille en est attaquée. Nous n'avons plus de contractions, ni de signes au galvanomètre."†

I therefore thought he had been fortunate enough to observe the negative variation before me, and accordingly I said, in my first paper, before relating the above-mentioned experiments: "Matteucci, in his 'Essai,' has stated that the frog current disappears DURING the tetanus."

By saying this, however, I was far from doing justice to myself.

* In the Report of the Committee of that Academy, concerning my Researches, it is said: "*Ce fait fondamental* (the negative variation) *donne une explication directe de la contraction induite de M. Matteucci.*"—"Comptes-rendus," &c. 15 Juillet 1850. t. xxxi., p. 36.

† "Bibliothèque universelle," &c. Mars 1838. Nouvelle Série. T. xviii., p. 164. "Essai," &c., pp. 81, 82.

The following quotations will show most clearly that Signor Matteucci, when writing the passage of the "Essai" just alluded to, and even a long time after the publication of my first paper, had not the slightest idea of the negative variation. He himself, indeed, has since given quite another interpretation of that passage.

In his paper of September, 1845, in which he first attacked me, he says, with regard to my explanation of the secondary contraction, with which he had become acquainted through Jourdan's French translation of John Müller's "Elements of Physiology":—" *M. du Bois-Reymond pense que le courant musculaire est interrompu par la contraction, et que cette interruption doit déterminer une rupture d'équilibre dans le nerf de la seconde préparation, et par conséquent, amener la contraction du second muscle. Ce sont les mêmes expressions avec lesquelles M. Müller rapporte l'hypothèse de M. du Bois-Reymond, faite pour expliquer la contraction induite. Il m'a été impossible de me faire une idée de la valeur physique de ces expressions. IL PARAÎT QUE M. DU BOIS-REYMOND ADMET QUE LE COURANT MUSCULAIRE OU PROPRE S'AFFAIBLIT OU S'INTERROMPT PENDANT LA CONTRACTION MUSCULAIRE ; MAIS IL N'Y A AUCUN FAIT QUI VIENNE A L'APPUI DE CETTE IDÉE.*"

Signor Matteucci, therefore, in September, 1845, pronounced that the fact of the negative variation, most clearly and explicitly stated in my first paper, was an hypothesis gratuitously invented by me in order to account for his so-called "Induced Contractions !"

Signor Matteucci continues:—" *Galvani avait bien vu que les signes des contractions propres s'affaiblissent ou disparaissent dans la grenouille prise de tétanos : moi-même j'ai bien confirmé ce fait ; mais il faut remarquer que cet affaiblissement se montre, parcequ'on prend pour indication du courant propre la contraction de la grenouille même, en repliant sa jambe sur les nerfs lombaires. MAIS ON NE TROUVE PAS CELA EN MESURANT LE COURANT PROPRE OU LE MUSCULAIRE AVEC LE GALVANOMÈTRE. La différence est donc due à l'état d'excitabilité du nerf dans l'animal tétanisé. Ce n'est que dans un cas, que j'ai noté dans mes premiers travaux et que j'ai vérifié après, que l'on pourrait trouver la preuve du principe admis par M. du Bois-Reymond. J'ai trouvé que les grenouilles prises dans l'état de surexcitation*

développée par l'emploi de la noix vomique, préparées à la manière ordinaire et disposées en pile, donnent un courant propre plus faible que celui que l'on obtient en agissant sur des grenouilles qui n'ont pas subi l'action de la noix vomique. Mais, si l'on réfléchit QUE LES CONTRACTIONS NE PERSISTENT PAS DANS LES GRENOUILLES PRÉPARÉES ET DISPOSÉES EN PILE, on ne pourra pas voir, dans ce fait unique, la démonstration du principe invoqué par M. du Bois-Reymond."*

Hence it may be seen that Signor Matteucci's statement, in the "Essai," concerning the influence of the tetanus on the muscular current, referred to quite another subject than at first glance it seemed to do. That statement, in truth, is explicitly acknowledged by its author solely to refer to the state of the current AFTER the tetanus has ceased. Signor Matteucci only wanted to say that the current of a frog, the muscles of which are exhausted by previous tetanic contractions, proves weaker than the current of a fresh frog, as might have been easily foreseen.

If after this any doubt could be entertained as to the true import of the passage of the "Essai," it would be removed by the statement, that during the years 1843—50, subsequent to the publication of my first paper, Signor Matteucci has made repeated attempts to detect some electric phenomenon in the muscles during contraction, without ever alluding by a single word to the passage of the "Essai" as bearing upon the same subject.

In January, 1843, Signor Matteucci wrote in the "Comptes-rendus"—"*Les signes du courant propre de la grenouille, démontrés par le galvanomètre, AUGMENTENT au même instrument dans l'acte de la contraction.*"†

In his "Traité," &c., published in 1844, Signor Matteucci repeated the same statement, and more fully detailed the experiments which led him to it. His object in trying these experiments was chiefly to account for the secondary contraction, to which, at that time, he was still inclined to ascribe an electric origin. He prepared ten frogs after Galvani's method—that is,

* "Annales de Chimie et de Physique." Septembre 1845. 3^e Série, t. xv., pp. 69, 70.

† 23 Janvier 1843. t. xvi., p. 197.

leaving the sciatic plexuses and a piece of the spinal column. He put them into the circuit so as to form a pile; the piece of the spine of each of the frogs rested on the feet of the following one. When the needle had taken its new position of equilibrium, under the influence of the frog current, Signor Matteucci excited contractions by wetting the nerves with a solution of caustic potassa. He says—“*Aussilôt l'alcali appliqué, les contractions commencent et l'aiguille, dans le même temps, s'avance de 5°, 6°, et quelque fois de 10° au delà de l'angle où elle était fixée. Quelques secondes après les contractions cessent, et l'aiguille revient à sa position pour descendre ensuite lentement vers le zéro comme elle l'aurait fait sans la déviation extraordinaire. Il faut avouer que l'apparition de deux phénomènes, c'est-à-dire, la production des contractions et la plus grande déviation de l'aiguille, se correspondent exactement. . . . Est-il maintenant bien exact de conclure que le courant propre augmente par la contraction du muscle? Peut-on expliquer différemment le résultat de nos expériences?*”*

Signor Matteucci, indeed, and with good reason, felt uneasy about the question whether the increase of the current at the moment of the contraction was not simply due to the decrease of the resistance of the circuit caused by the alkali; and as he did not succeed in devising a proper method for avoiding that cause of error, he at last despondingly concludes:—“*Je n'ose pas affirmer que la question soit complètement résolue, et je me suis arrêté, ne sachant pas par quelle voie avancer pour la résoudre.*”†

In the same work, however, the statement of the “Essai” is also reprinted, in which the tetanic convulsions are said to impair the intensity of the frog current,‡ without any connexion whatever being understood to exist between the two statements; and in fact no such connexion existed in their author's mind, for the prior and seemingly correct statement of the “Essai” referred only to the state of the current after the tetanus had relaxed; the more recent and erroneous statement, on the contrary, referred to the state of the current during the act of contraction itself.

In his paper “On Induced Contractions,” printed in the “Philosophical Transactions” for 1845, Signor Matteucci resumed these experiments, and finally succeeded in satisfying himself that the increase of the current formerly observed by him to

* “Traité,” &c. 1844, p. 138.

Ibid. p. 141.

‡ Ibid. pp. 109, 110.

take place during the contractions was only due to the decrease of resistance caused by wetting the nerves with alkali. He therefore then says—“*Let us then conclude that direct experiment answers negatively to the question we proposed to solve, whether there were evolution of electricity in muscular contraction.*”^{*} And this statement he has since repeated several times—first, in his letter to M. Dumas, of September, 1845:—“*J’ai varié de mille manières les expériences pour découvrir s’il y a un dégagement d’électricité dans la contraction musculaire. J’ai employé pour cela les instruments les plus délicats et tous les soins possibles, et je dois conclure que ce dégagement d’électricité ne peut pas se démontrer par l’expérience;*”[†] then, in 1846, in the French translation of the paper of the “Philosophical Transactions,” published in the “*Annales de Chimie et de Physique*,”[‡] again, in 1847, in a letter to M. Dumas, printed in the “*Comptes-rendus*”:—“*On ne trouve aucune augmentation dans le courant musculaire pendant la contraction du muscle;*”[§] and, once more in the same year, in his “*Leçons sur les Phénomènes physiques des Corps vivants*,” p. 277:—“*Concluons donc que l’expérience directe répond négativement à la question que nous nous sommes adressée s’il y avait développement d’électricité pendant la contraction musculaire.*” Lastly, in 1849, after I had given a short account of my experiments on the negative variation as observed on the living body of man, Signor Matteucci, who in vain attempted to repeat these experiments, wrote in the “*Comptes-rendus*”:—“*C’est avec le plus vif intérêt que j’ai répété cette expérience. J’avais espéré qu’elle me tirerait une fois pour toutes de l’incertitude dans laquelle je suis depuis la découverte de la contraction induite. Malgré un très-grand nombre d’expériences, dans lesquelles j’ai tâché de découvrir la vraie nature de la contraction induite, j’ai dû à la fin conclure qu’il m’était impossible de décider si la cause était un dégagement d’électricité par la contraction, ou un véritable cas d’induction nerveuse . . . J’ai donc bien démontré que, de l’expérience de M. du*

* “Philosophical Transactions for the year 1845.” P. ii., p. 307.

† “*Annales de Chimie et de Physique.*” Septembre 1845. 3^e Série, t. xv., pp. 67, 68. The reader will remember that in that same letter Signor Matteucci gave the true interpretation of the passage of the “*Essai*,” and ridiculed what he called my “*hypothesis*” of the negative variation.

‡ “*Annales de Chimie et de Physique.*” Octobre 1846. 3^e Série, t. xviii., p. 122.

§ 15 Mars 1847. t. xxiv., p. 415.

*Bois-Reymond, il ne résulte pas la preuve du développement de l'électricité par la contraction musculaire."**

The failure, indeed, of all his endeavours to discover some electric phenomenon connected with muscular contraction, greatly contributed to make him reject the electric origin of the secondary contraction, and to mislead him into that strange hypothesis of a nervous induction, by which he has accounted for that phenomenon, and to which he still seems to adhere.

§ XI. SIGNOR MATTEUCCI'S CLAIMS TO THE DISCOVERY OF THE
NEGATIVE VARIATION OF THE MUSCULAR CURRENT
DURING CONTRACTION EXAMINED.

Such, then, were Signor Matteucci's successive statements on the electric action of muscles during contraction. Setting aside the interpretation which he afterwards gave of the passage of the "Essai," he successively asserted—

1. That the muscular current disappears during the contraction.
2. That the muscular current increases during the contraction.
3. That the muscular current remains unchanged during the contraction.

By the two latter statements, Signor Matteucci most evidently would have destroyed his claim to the discovery of the negative variation to which the first statement might have entitled him, if he himself, in September, 1845, had not given quite another interpretation of that first statement.

As late as 1849, Signor Matteucci maintained that no electric phenomenon is exhibited by the muscles during contraction. In 1850, however, the Report of the Committee of the Académie des Sciences no longer allowed him to doubt the accuracy of my experiments, and the insufficiency of his own. He therefore since admitted the correctness of my statement concerning the negative variation, although he still denied the possibility of accounting by it for the secondary contraction.† And ulti-

* "Comptes-rendus," &c. 25 Juin 1849. t. xxviii., p. 783.

† "Comptes-rendus," &c. 3 Février 1851. t. xxxii., pp. 131, 132.

mately, Signor Matteucci, in his third attack upon me, ventured to claim the discovery of the negative variation on the ground of the above-mentioned passage of the "Essai."*

In my reply, I opposed him almost literally with the same arguments which I have used above. I convicted him, by literal quotation of his own words, of having since given quite another interpretation of the passage in question, and of having, during seven years after my publication of the negative variation, repeatedly stated, first, that the contraction is attended with an increase of the current, and afterwards, that no change whatever becomes perceptible in the electric condition of muscles during contraction.

Signor Matteucci, however, without mentioning one word regarding my reply, in his last pamphlet, once more brings forward the same assertion of priority. He says, at p. 4: "*Parmi mes expériences de cette époque (de 1838 à 1842) sur le courant propre, il y a celles qui prouvent évidemment les modifications souffertes par le courant de la grenouille sous les contractions tétaniques ;*" and, at p. 15: "*Lorsque, longtemps après la publication de ce Mémoire, j'appris par l'édition française du 'Manuel de Physiologie' de M. Müller, que ce Mémoire existait et que j'ai pu en lire une traduction, j'y ai trouvé, comme il m'arrive toujours, et à part les vues hypothétiques, qu'il avait vérifié mes expériences sur le courant musculaire ; QU'AVEC LE GALVANOMÈTRE IL AVAIT OBSERVÉ UNE DIMINUTION DANS CE COURANT PENDANT LA CONTRACTION, CE QUI CONFIRMAIT UNE DE MES ANCIENNES EXPÉRIENCES,*" &c.

Thus, in September, 1845, Signor Matteucci pronounced my discovery of the negative variation a mere hypothesis, and declared that the passage of the "Essai" solely related to the state of the current AFTER the tetanus had relaxed. But, as soon as he could no longer question the truth of that discovery, he stated that he never took it for anything but a mere confirmation of his own statement in the "Essai," which he then asserts to refer to the state of the current DURING the tetanus. I shall abstain from drawing any conclusion from these facts.

But the following passage of the pamphlet yet calls for some

* "Comptes rendus," &c. 3 Juin 1850. t. xxx., p. 705.

remarks : “ *Vous auriez été plus juste et plus exact en disant, que n'ayant pas un galvanomètre aussi sensible que M. du Bois-Reymond, je n'avais pu réussir, par des expériences tentées sur des piles musculaires en contraction, à confirmer le fait que j'avais vu précédemment en opérant sur la grenouille entière tétanisée.*” (p. 12.) Signor Matteucci, therefore, ascribes his inability to observe the negative variation to the want of a sufficiently sensitive galvanometer. In expressing that opinion, he no doubt forgot what he had written a few pages before : “ *M. du Bois-Reymond a certainement rendu un service à la science, puisqu'il a réussi à donner au galvanomètre une si grande sensibilité : mais je le répète, un tel instrument n'était pas nécessaire pour étudier le courant musculaire et ses lois, car toutes mes expériences ET TOUTES CELLES QU'ON PEUT FAIRE SUR LES MUSCLES, réussissent très bien avec des galvanomètres ordinaires et avec des piles musculaires.*” (p. 6.)

But Signor Matteucci is altogether mistaken with respect to this point. With his galvanometer of 2500 coils of wire, I will undertake to show the negative variation most manifestly, provided, of course, I be allowed to use those methods of experiment which are so despised by Signor Matteucci. The galvanometer with which I first observed that phenomenon is that described in my first paper, being of 4560 coils of wire only. (See above, p. 11, Note 1.) On the other hand, Signor Matteucci might have at his command the most sensitive of those magnificent apparatus, recently made at Berlin, by M. Sauerwald, under my direction, and having from 25,000 to 30,000 coils of wire, and, unless he should consent to exchange his methods for mine, he will remain quite as unable as ever to repeat my experiments.

Be this as it may, Signor Matteucci, in the passage under consideration, desires to be excused, on account of the insensibility of his galvanometer, for not having repeated, with muscular piles, the observation which he says he made upon a single frog. But it is hard to conceive why a single frog should give a stronger current than a pile of twenty half thighs of frogs ; and, if it did, Signor Matteucci was at perfect liberty to repeat the observation, related in the “Essai,” upon a single frog, instead of using a muscular pile.

At all events, Signor Matteucci, in that passage, clearly ad-

mits what was sufficiently evident without that admission, from the whole of the above statements,—namely, that, since that observation to which, in September, 1845, he most unequivocally ascribed quite another import, he never saw anything like a negative variation of the muscular current on contraction. Yet he claims the discovery of that negative variation. So we have here an instance, unprecedented, I believe, in the history of science, of a claim being laid to the discovery of a phenomenon, which the claimant never saw, and, perhaps, never will see.

CONCLUSION.

There are many other points in Signor Matteucci's pamphlet which suggest similar remarks to me. But I am afraid the patience of my readers will hardly have been equal to the prolixity of the preceding detail. So I only beg to declare, that if I forbear from remonstrating against any other passage in that pamphlet, it is by no means because I submit to its meaning, or because I am unable to confute it with arguments quite as unanswerable as those adduced above.

Signor Matteucci, at the end of his pamphlet, promises that this attack upon me shall be the last one. Nobody, indeed, will be more glad of this determination than I; but to nobody, perhaps, will it be more profitable than to Signor Matteucci himself.

The new edition of his "Traité," which he is going to publish, will afford him an opportunity of at last turning to account the observations on his claims contained in my successive replies. As regards that new edition, however, I cannot refrain from expressing to Signor Matteucci some apprehension about his design to review the whole of my researches in it. (See above, p. 8.)

Signor Matteucci's own researches in animal electricity, as far as they have been successful, and setting aside his valuable papers on the torpedo, have hitherto only extended to some particular cases of the muscular current, the general law of which he still seems not to understand aright; to the influence which several circumstances, generally of a deleterious or injurious character, exert upon that current; and, lastly, to the secondary

contraction, which he discovered, but did not succeed in tracing to its true origin. Now a mere inspection of the table of contents of the "Abstract," or of the short conclusions enumerated in its last chapter, will suffice to show that I have enlarged our knowledge of this class of phenomena, far beyond the point at which Signor Matteucci arrived.

From the preceding pages it might appear, indeed, as if I had only worked at the same subjects as Signor Matteucci. But this is only because in his pamphlet he confined himself to an examination of those passages of the "Abstract" in which his name happened to occur (pp. 3, 14). Thus, of course, all those parts of that book escaped his censure, and consequently were passed over in silence, in which subjects utterly unknown to him are treated of, as in such parts there was no occasion to mention him.

These parts, however, form rather the greater part of the "Abstract." The statements thus far adduced in the present reply were mostly taken from my paper of January, 1843. Since that time, up to the present, I have incessantly and exclusively been employed in prosecuting these researches. Of the results which I have obtained I will only mention the following here.

To the law of the muscular current, as it was stated by me in my first paper, I soon added the law according to which different points also of the longitudinal or transverse section alone produce currents on being connected by the galvanometer wire (Abstract, p. 92); then the law, according to which the intensity of the muscular current varies with the position of the muscle on the ends of the galvanometer circuit, whether the distance of these ends remain unchanged or be allowed to vary also; again, the law according to which that intensity varies with the length and thickness of muscles (p. 115). The same laws I proved to apply to the nerves (pp. 164, 172).

I examined more closely into the electric action of the natural transverse section of muscles, and succeeded in explaining all its apparent anomalies by pointing out what I have called the *parelectronic* layer of the muscle, that is, a layer of muscular substance situated beneath the expansion of the tendon, the electromotive power of which more or less counteracts that of the rest of the muscle (p. 116). I showed that the electromotive power of contractile tissues

varies in the same ratio as their mechanical energy (p. 161). I determined the electric action of the skin of the frog and of man,* and by the results of this investigation I succeeded in demonstrating the muscular current during the state of rest of the muscles, even on the live undissected body, at least in frogs (p. 125). As to the electric condition of muscles during contraction, my principal results are already mentioned above, at page 28. I traced the nervous current through the whole nervous system, and through the whole series of animals (Abstract, p. 169). The electromotive power of ganglia I demonstrated on the abdominal ganglia of the crab. I pointed out those highly remarkable modifications of the electric deportment of nerves when under the influence of an electric current, which I designated by the name of the electrotonic state of nerves (p. 174). The negative variation, which I showed that the nervous current underwent on tetanising the nerves, has been also alluded to already, at page 28. Indeed, I succeeded in 1847, after four years' toil, in transmuting into a slight motion of the magnetic needle, those internal motions of the nerves which, on reaching the muscles, would have caused convulsions, on reaching the brain, would have aroused sensation (p. 187—193). Since the middle of the last century, some result of this kind had been longed for by physiologists, and consequently aimed at in a great many fruitless experiments.

I omit several other points of no less importance here, because their examination does not (or at least not so indispensably as that of the points just alluded to) require those peculiar means of investigation devised by me twelve years ago, and since fully detailed in my German work. The above stated results, for the most part, can only be obtained by means of a galvanometer of high sensitiveness, of the conducting vessels with their cushions or some similar contrivance, and of that host of little auxiliary apparatus well known to those who ever watched my experiments. They cannot be obtained, in short, by means of an ordinary galvanometer, and of piles of muscles or nerves, for the very simple reason that the conditions necessary in order to obtain them generally are so delicate, that they often are hardly

* "Monatsberichte der Berliner Akademie, März 1852," S. 111.

to be fulfilled, even on a single muscle or nerve, and that, accordingly, it would be impossible to do so on twenty muscles or nerves at once. Thus, it is perfectly obvious to any one acquainted with this subject, that Signor Matteucci's repeated attempts to discover a variation of the muscular current during contraction (without mentioning several other circumstances) could not but fail, because of the impossibility under which he laboured of making the ten frogs which composed his pile contract at the same time.

Signor Matteucci, however, in his last pamphlet, asserts, that his method of experimenting on twenty half thighs of frogs is the only commendable one:—" *Je soutiens, ce qui d'ailleurs est bien évident pour tous ceux qui n'ont pas d'intérêt contraire, que la méthode des piles musculaires est la seule vraie. . . . Personne au monde qui pourrait examiner de ses propres yeux la forme et les qualités d'un objet de dimensions assez grandes, se donnerait la peine de le réduire très petit pour l'observer avec une loupe*" (p. 6). Signor Matteucci's comparison is very good indeed, but it is wrongly applied, for there are many things here, and very important too, which, to continue his simile, cannot be seen with the naked eye, but require the use of a most powerful magnifying apparatus in order to be perceived. Yet Signor Matteucci condemns and rejects my methods altogether, by which alone for the present such means of magnifying are afforded, and he does not in the least seem inclined to employ them for repeating my experiments.

Signor Matteucci, therefore, is an utter stranger to all those results, and he will necessarily remain so. In consequence, I am sorry to say, I am compelled to QUESTION HIS COMPETENCE AS A JUDGE OF THE ACCURACY OF MY STATEMENTS; and I protest beforehand against the results of the discussion to which he says he will submit my experiments and theory in the new edition of his "*Traité*," unless, in the course of the trial, he furnishes sufficient evidence that he has followed minutely the directions given by me in my German work for repeating my observations.

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